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A REPLY

TO THE REVIEW OF

DR. DRUMMOND'S

FIRST STEPS TO ANATOMY,

CONTAINED IN

The British and Foreign Medical Review,

FOR APRIL, 1846.

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REPLY.

In September, 1845, I published a little volume, entitled, "First Steps to Anatomy," in the preface to which, I stated that my principal aim was, "to prepare the young medical student, by some initiatory broad views of the general component parts of the animal frame, that he may thereby be better enabled to enter on the innumerable and minute details of physiology and descriptive anatomy." And I farther stated, that I had written it "in what I conceived to be somewhat of a popular style," and that I offered it to the public, "in the hope, perhaps a vain one, that it might prove of interest, not to the medical student alone, but to the general reader." These were, certainly, not very lofty pretensions; and I could not but feel some surprise, that in a notice of the little book, in the "British and Foreign Medical Review," for April last, I had incurred the censure of the reviewer, in no trifling degree, the volume being declared by him as decidedly behind the present state of science, an accusation amounting, as I conceive, if not directly, at least virtually, to a charge of incompetency. This notice I read hurriedly, a few days before the month of May, but I was then too much occupied with dredging tackle, sketching materials, packing up books, and other preparations for spending the summer at the sea-side, to let it occupy either my thoughts or attention; and it is only of late that I have carefully examined the reviewer's statements, which being, I think, wrong in every point he advances, I feel imperatively called on, in justice to my own character, as a teacher, to give them a reply. I will first give the entire article from the "Review," which is as follows:—

"ART. I.—*First Steps to Anatomy.* By JAMES L. DRUMMOND, M.D., Professor of Anatomy and Physiology in the Royal Belfast Institution.—*London*, 1845. 12mo, pp. 201. With Twelve Plates.

"The *idea* of this little volume is excellent; its aim being 'to prepare the young medical student, by some initiatory broad views of the general component parts of the animal frame, that he may thereby be better enabled to enter on the innumerable and minute details of physiology and descriptive anatomy.' In many respects the execution is correspondingly good. The style is very simple, as befits the object; and the general arrangement of the subjects well fitted to lead on the student by progressive steps towards the higher and more difficult departments of anatomical and physiological science. The chief fault we have to find in it lies in this: that it is, in many respects, decidedly behind the present state of science. We see no more reason why this fault should be excused in an elementary work than in one intended for the more advanced student; since it appears to us to be above all things essential that those who are entering upon any department of study should be made to *learn* as little as possible what they may afterwards be called upon to unlearn. We must justify our statement by a few extracts:

"'While the principle of life thus resists the decomposing operations of chemistry, we must recollect that it is only the injurious part of chemical action which is resisted, and so far is life inimical to this action, that it is only when life itself would be endangered that it is opposed and forbidden; for many chemical processes are carried on in the living body. Among these I may mention respiration and digestion; but these are all for good, and those chemical affinities only, which would tend to injure or destroy the organization, are forbidden to exert their power.'

"We have on various former occasions endeavoured to expose the fallacy of this doctrine (a very *prevalent* one we admit it to be, but not the more true or philosophical), that the vital power in any way controls chemical change; and the evidence is continually becoming more definito and irresistible, that all these peculiar powers termed vital (and we may instance those of the muscular and nervous systems as most unequivocally falling under this category) are dependent for their manifestation upon chemical changes, which are destructive to the tissues exhibiting them; just as the manifestation of electric power in a voltaic battery is dependent upon the chemical changes taking place between the metals and fluids in its cells. We shall not now dwell further on this topic, since we could scarcely do so without entering upon the question *in extenso*, which we have already discussed as fully as we deem suitable to the nature of the subject.

"In the second chapter we have our old friend the cellular substance; which we are informed, as of old, constitutes 'the basis of every solid part of an animal.' Surely Dr. Drummond must be aware that anatomists, in this country at least, have now universally agreed to change the old designation of the tissue for one which more satisfactorily expresses its character—namely, *areolar* tissue; and that the real celluar tissues of the body, whose extent and actions have been ascertained by the aid of the microscope, perform functions of far greater importance. The functions of the true areolar tissue we conceive to be purely mechanical; it ties together the different elements of other tissues, in such a manner as to permit them to have a certain degree of movement upon one another; but of its being 'the matrix or mould in and by which every structure is imbedded, pervaded, or enclosed,' we believe that there is an utter deficiency of proof. In cartilage, for instance, not a trace of it is to be found. In bone it only lines the medullary canals and cancelli, serving to fix the vessels in their places; in teeth it does not extend beyond the pulp cavity, where it answers the same purpose; and there are many other textures of which the same may be said. Nor is it correct to say that it forms the chief bulk of the tendons and ligaments, since, as Messrs. Todd and Bowman have shown, the areolar tissue contains an admixture of two elements—the *white* and the *yellow* fibrous structures; of which the former alone is found in the tendons and non-elastic ligaments, whilst the latter is found only in the elastic ligaments. Again, in the succeeding chapter on fat, not a word is said of the important office to which this tissue is subservient in the living body—that of serving as a reservoir of combustible material, by which our heat is kept up, when from any cause there is a suspension of the supply afforded by the food. The experiments of Chossat prove that, if this reservoir be entirely exhausted, the animal is dependent, from hour to hour, upon the combustible matter ingested and absorbed; and thus, if it were not for such a provision, the loss of a single dinner, to say nothing of a longer abstinence, would be fatal. Some little notice of the more precise views, on these and other subjects, which have of late contributed to raise physiology to a much higher rank among the sciences, would greatly improve the value of Dr. Drummond's treatise, in this and many other departments; and such we hope to see introduced in another edition.

"In a concluding chapter, entitled 'Thoughts on Anatomical Education,' Dr. Drummond urges what has often been suggested by ourselves as likely to prove advantageous—that the entire first year's course should be of a general preparatory nature, embracing no one subject in detail, but of a nature to lay a broad and solid foundation for the more special inquiries to be subsequently pursued."

"In the second chapter (says the reviewer) we have our old friend the cellular substance, which we are informed, as of old, constitutes 'the basis of every part of an animal.' Surely Dr. Drummond must be aware that anatomists, in this country at least, have now universally agreed to change the old designation of the tissue for one which more satisfactorily expresses its character—namely, *areolar* tissue; and that the real cellular tissues of the body, whose extent and actions have been ascertained by the aid of the microscope, perform functions of far greater importance."—p. 494. Now, in answer to this remark, I will say, that the introduction of the new term "*areolar tissue*," was quite uncalled for, and has been brought forward most unnecessarily, to supplant terms which, for very long periods of time, have been in use, perfectly well understood, and universally adopted.

We do not all like to see "old friends" with new faces, though, at the present time, there is such a general verbomania, that, in some branches of science, one would almost require a new lexicon annually to explain the multitudes of novel terms brought forward, many of them more objectionable than the old, most of them perfectly unnecessary, and not a few of them absolutely absurd. This is especially the case in every branch of natural history; but the infection has long since spread, and is now spreading still more rapidly, to the medical sciences. This is more particularly conspicuous in the multiplicity of names applied to lesions of the eye and its appendages; and hence it has been well observed, that "in fact it would require years of study to become acquainted with the mere synonymes which have been heaped up to such a Babelish height by the writers on eye-diseases."*

But it is not in one only, but in every branch of medicine, that this verbomania is making rapid strides; and nothing can be more ridiculous than the reasons assigned, in some instances, for changing old established names into others of new coinage. This is very happily illustrated, in the following example, by Dr. Watson, in his excellent Lectures on the Principles and Practice of Physic—*Ed.* 2, *vol.* 1, *page* 387. Speaking of delirium tremens, he says:—

> "There is delirium always; and there is generally, but not always, tremor. The name is a good enough name, in my humble opinion; yet it has been found fault with, because the trembling is not in all cases present; and some have, therefore, christened it *delirium è potu*, or *delirium ebriositatis*. But these terms are open to just the same objection as the other; for, though the disorder is most commonly connected with intemperate habits, that is not always the case. One very curious fault has been discovered in the name: it is said that the delirium cannot *tremble;* and, therefore, that it is better to say *delirium cum tremore*, or *tremefaciens:* and you would hardly suppose it, but there has been a sort of contention for the honour of thus mending the nomenclature of this disease. But they who object to *delirium tremens* appear to see no harm in *delirium ferox;* whereas, it is just as incorrect to say *delirium* is fierce, as to say that it trembles: it is the patient who is furious, even as it is the patient who trembles; and all this dispute about a name is mere trifling. It matters not what we call a disease, so that the name conveys no erroneous theory as to its nature or treatment. No such source of error attaches itself to the term delirium tremens; and, therefore, if it be only to avoid the inconvenience of change, we will adhere to that term."

I will now revert to the reviewer's remark, that—"Surely Dr. Drummond must be aware," &c. Dr. Drummond is not aware of any such thing, and he is glad to find that the cellular substance, and its synonymes, cellular tissue, cellular membrane, and cellular texture, are adhered to by most of the highest names in the profession; and he believes that, in surgical works, they never can be dispensed with. These terms, indeed, should continue to be considered as almost sacred, were it only from their having been used by the most illustrious men in that profession for more than the last hundred years. The term areolar tissue, instead of being *universally* adopted by British anatomists, is, I believe, countenanced by only a very small section of them; the reviewer, however, asserts that it has been universally adopted by the anatomists, at least of this country. Now, I will test the assertion by the contents of this forty-second number of his "Review." We find in it, for instance, a very favourable notice (and, indeed, no other could be given) of the second edition of Mr. Fergusson's System of Practical Surgery. In this excellent work, independent of the description of the various operations on the human body, there is an accurate description also of the anatomy of such parts as may become the subjects of surgical operation; and here, if anywhere (in a work published in the present year) were the term *areolar tissue* universally adopted, surely we would expect to find it. I have gone most carefully over the whole work, consisting of 647 pages; but in the entire number the term *areolar tissue* is not even hinted at. Every student of medicine knows that the terms, cellular membrane, cellular substance, cellular texture, and cellular tissue, are all synonymous; so that, when we use any one of these, it is the same as if we had used any one of the others. Now, in this work I carefully noted down the number of times in which these synonymes have been used, and they stand thus:—

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* British and Foreign Medical Review, vol. v., page 43.

Cellular membrane, 24 times;

Cellular tissue, 27 times;

Cellular texture, 41 times; and our "old friend" the cellular substance, 9 times; while the term, areolar tissue, is never, as I have said, mentioned from first to last.

But do not the reviewer's colleagues adopt the new term, if it be, as the writer asserts, universally adopted? Not at all; in this forty-second number of the "Review," forming half a volume, it occurs only twice—once by the reviewer, in his bold assertion, and once by Mr. Paget, in his Report, *page* 566. The term, areolar tissue, is never mentioned in any other part; but we find one or more of its old and legitimate names adopted by the reviewers themselves; and hence we have the following, not in quotations from other works, but in the Reviewer's own words:—" The cicatrix only should be divided, not any of the surrounding healthy *cellular tissue," Reviewer, in page* 290—"thickened *cellular tissue," Do. page* 300—"then the hard cicatrix is completely removed, the *cellular tissue* of the under border of the scrotum loosened," *Do. page* 313—"covered by a fine ash-coloured coat of mortified "*cellular tissue," Do. page* 316—"even if torn muscle or *cellular tissue* render it necessary," *Do. page* 324—"thickening of the submucous *cellular tissue," Do. page* 326—"surrounded by a pseudo-cyst of condensed *cellular tissue," Do. page* 423—"the intermixture of ordinary *cellular tissue," Do. page* 426. Dr. Laycock, in a letter to the editor, mentions the "mucous or *cellular tissues," page* 527; and Mr. Paget, who writes the Report on Anatomy and Physiology, mentioning Mr. Goodsir's observations on the structure of bone, says, *page* 548, "He also describes the tissue which lies between the blood-vessels and the walls of the Haversian canals as a layer of *cellular substance,* at least in the fœtus, for in the adult it is replaced by fat." The term, cellular tissue, occurs several times in the quotations: that of areolar tissue, not once.

The next part of the reviewer's notice which I will advert to, is that in which he says of the cellular (or, if he will, the areolar) tissue, that "in cartilage not a trace of it is to be found." Now, I would ask, is he *quite* sure that cartilage itself is not a modification of the self-same tissue? For my own part, I care not whether or no; but I think the following extracts, from one of the most celebrated of modern pathologists, lend at least (in spite of the microscope) a very strong probability to the opinion that it is. "There are few parts," says Andral, "of the cellular tissue which is interposed between the different organs, or tissues, in which the development of accidental cartilages has not been observed. Thus, for instance, the cellular tissue which is subjacent to serous membrane is frequently the seat of these productions. Cartilage has been found in the form of isolated grains, large spots, and irregular incrustations; in the cellular membrane which lines the arachnoid, the pleura, the pericardium, the peritoneum, the tunica vaginalis, and the synovial membranes. In these different situations accidental cartilages may be found—first, unattended by any other appreciable lesion; second, co-existing with different alterations of the serous membrane under which they are formed, such as adhesions, effusions, &c.; third, along with various morbid changes in the substance of the organs which are enveloped by the serous tunic. In many cases of this description it is possible to follow the different degrees of modification which the texture of the cellular tissue undergoes during its transformation into cartilage. For example, behind the same pleura it is by no means uncommon to observe some spots in which the cellular tissue is condensed and indurated, but still retains its natural organization; others in which it is become evidently fibrous; and lastly, others in which it presents merely a homogeneous white mass, the texture of which is perfectly analogous to that of cartilage."*

There is considerably more to the same purpose; but I will only extract the following:—"It is exceedingly rare to find any tissue, except the cellular, undergoing a real transformation into cartilage."† But, without admitting that cartilage is a modification of the cellular substance, is the reviewer fairly warranted in saying that "not a *trace*" of the latter is to be found in it? I would ask him, is not the purest cartilage composed of corpuscles deposited in an intercellular substance, and is not each corpuscle invested with a coat or covering of the same? I will not assert that this is real cellular tissue, but all analogy would lead us to that conclusion; and if it be not, can the reviewer tell us what it is?

"Nor is it correct (says the same authority) to say that it forms the chief bulk of the tendons and ligaments, since, as Messrs. Todd and Bowman have shown, the areolar tissue contains an admixture of two elements—the *white* and the *yellow* fibrous structures, of which the former alone is found in the tendons and non-elastic ligaments, whilst the latter is found only in the elastic ligaments." I have no inclination to doubt for a moment the correctness of the observations of these gentlemen; but I must positively deny the assertion of the reviewer, that this tissue does not form "the chief bulk of the tendons and ligaments," and that, even on his own showing. Admitting, with him, that the areolar tissue consists of two materials—one white, and the other yellowish—is it not obvious, that if the white part alone enter into the composition of any organ, or if only the yellow part enter it, still it is *the* areolar tissue? But I will ask, how are the white fibres of the tendon connected to each other? From what we know of the cellular substance, as being the common connecting medium of the different organs of the body, we would naturally infer, that they are joined to each other by *it*. *And so they are.* The firm white fibres are united to each other by the cellular tissue in its reticular, though dense, form; and hence, with the exception of a few vessels entering their composition, not only the *chief*, but the *entire*, bulk of the tendons is formed of cellular substance; and the same statement will apply also to the ligaments. If the reviewer should ever look into the Cyclopædia of Anatomy and Physiology, I would recommend to his particular attention the following passage, by Mr. Grainger, in the article, "Fibrous Tissue:"—"The result of a careful examination proves, that the remarkably firm and resisting threads which constitute the basis of the various fibrous organs, are composed of condensed cellular tissue. In certain regions we may perceive the gradual transformation of the cellular tissue into a fibrous organ, as in the formation of the superficial fascia of the abdomen; whilst, by prolonged maceration, *the most dense tendon or ligament may be reduced into a pulpy cellular substance.* This opinion is corroborated by Isenflamm, who conceives that this tissue is formed by cellular fibres, impregnated with gluten and albumen; and also by Beclard, who regards it as being composed of *cellular texture, very much condensed.*"*

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* Andral's Pathological Anatomy, translated by Drs. Townsend and West—vol. 1, page 338.

† *Ibid*, page 346.

The reviewer again says, that in bone the areolar tissue "only lines the medullary canals and cancelli, serving to fix the vessels in their places." This I will believe, when he shows that the bones do not abound in blood-vessels, and that cellular tissue does not enter into the composition of the latter—but not *till* then.

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* "The tendon, then, is nothing more than the cellular membrane, which is in the interstices of the muscular fibres, condensed together. The tendinous origin of a muscle, for example, may be traced through the muscle from one extremity to the other, till it is again gathered and twisted into the tendinous insertion. They may be resolved into loose cellular membranes by maceration, and many tendons may be stretched out into a web, even without maceration."—SIR CHAS. BELL.

I have to remark on what the reviewer calls the "real cellular tissues of the body," that when it came to be ascertained that the primordial state of the animal organs was, as in plants, vesicular, it was thought proper to take the name of cellular tissue from the substance to which it had been so long applied, and transfer it to this early stage of organization ; but, though not smitten with the verbomania so generally prevalent, I would be of opinion, that, had the term *cystic* been adopted, it would have been a much better appellation ; for, surely, cystic wall, cystic structure, cystic germ, cystic development, would be considerably more euphonical (and equally expressive of what was meant) than cell wall, cell structure, cell germ, &c. It has been found that these cells, when seen by the microscope, offer precisely the same appearance, whether from a plant or an animal, or whether from brain, bone, muscle, or any other tissue whatever ; and what is more, if this early state of development be similarly examined in the most deadly and malignant growths to which the human body is subject, such as cancer, or fungus hæmatodes, the primordial cells present exactly the same appearance as those do which commence the most natural and healthy structures ; and as to the mode in which they operate, or how they are the means of building up the system, of forming nervous matter in one place, muscle in another, and so on, we are in complete ignorance. As for saying, as has been done, that some cells attract the elements of fat, others the elements of brain, others those of bile, &c., it is but the show of knowledge, and can only be considered on a par with the curate of Brendon's theory of the tides—namely, that it was just their own natural course of so doing.

Though, along with the reviewer, I have recognised the white and yellow fibrous tissues as belonging to the cellular (or areolar), yet, I believe that, while the white fibrous tissue is no more than a modification or form of the ordinary cellular substance, with which it is identical, both in its microscopical and chemical characters, the yellow or elastic tissue is altogether different, and is only *superadded* to it in such structures as require a degree of elasticity and extensibility, which the cellular tissue by itself does not possess; and as the muscular fibres are connected to each other, and enveloped by cellular tissue, so, in a similar way, the fibres of the yellow tissue are connected, and surrounded by it; but, as the muscular fibre possesses properties totally different from its connecting medium, so also we may consider the yellow fibre equally distinct and *sui generis.*

I have now done with our "old friend," the cellular substance; and, "as of old," I still continue in the belief, that while "in the zoophyte the entire body appears to consist of the cellular tissue," that "even in man it enters so largely into the formation of the different organs, pervading equally the most delicate and the most solid parts, that it constitutes a species of mould of the whole body, and of its individual parts; indeed, if we except the enamel of the teeth, and, as some authorities contend, also the nails, the hairs, and the epidermis, there is *no solid* in which it may not be detected."*

"Again (says the reviewer) in the succeeding chapter on fat, not a word is said of the important office to which this tissue is subservient in the living body—that of serving as a reservoir of combustible material, by which our heat is kept up, when, from any cause, there is a suspension of the supply afforded by the food. The experiments of Chossat prove that, if this reservoir be entirely exhausted, the animal is dependent, from hour to hour, upon the combustible matter ingested and absorbed; and thus, if it were not for such a provision, the loss of a single dinner, to say nothing of a longer abstinence, would be fatal."

I will here quote Dr. Carpenter's account of Chossat's experiments, from his Manual of Physiology, which, in the same number of the "Review," is said to be the best manual extant:

> "But when there is a deficiency of the proper animal heat, the vital activity of the system may be maintained by caloric, supplied by external sources. This fact is of high scientific value, as giving the most complete demonstra-

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* Cyclopædia of Anatomy—Art. "Cellular Tissue."

tion of the *immediate* dependance of the vital functions of warm-blooded animals upon a sustained temperature; and its practical importance can scarcely be over-rated. It rests chiefly upon the recent experiments of Chossat, who had in view to determine the circumstances attending death by inanition or starvation. He found that, when pigeons were entirely deprived of food and water, their average temperature underwent a tolerably regular diminution, from day to day; so that, after several days (the exact number varying with their previous condition), it was about 4½° lower than at first. Up to this time, it seems that the store of fat laid up in the body supplies the requisite material for the combustive process; so that no very injurious depression of temperature occurs. But as soon as this is exhausted, the temperature falls rapidly, from hour to hour; and as soon as the total depression has reached 29½° or 30°, death supervenes. Yet it was found by M. Chossat, that when animals, thus reduced by starvation, whose death seemed impending (death actually taking place in many instances, whilst the preliminary processes of weighing, the application of the thermometer, &c., were being performed), were subjected to artificial heat, they were almost uniformly restored from a state of insensibility, and want of muscular power, to a condition of comparative activity. Their temperature rose, their muscular power returned, they took food when it was presented to them, and their secretions were renewed; and, if this artificial assistance was sufficiently prolonged, and they were supplied with food, they recovered. If the heat was withdrawn, however, before the time when the digested food was ready, in sufficient amount, to supply the combustive process, they still sank for want of it."—*Pages* 67–8.

If, in Chossat's cruel experiments, it had been found that the fat alone disappeared, and that, in proportion as it did so, the temperature sank, and at length died out, when the fat was exhausted, there would be some rational ground for supposing that it is the great supporter of animal heat; but we know that in starvation almost the *whole* substance of the body wastes; and the sinking of the temperature can be easily accounted for, by the diminished force of the circulation, and the general debility arising from the want of nourishment. So extreme was the point of weakness and depression of the living powers, that, as stated above, many of the animals died in the act of weighing them, or of applying the thermometer; but before arriving at this extremity, the application of artificial heat caused a temporary revival. Surely there is nothing in this that cannot be readily explained, independent of the question of fat altogether; for, when the powers of life were thus sunk to the lowest ebb, the addition of any strong stimulus would, for a time, arouse them to some increased activity, just as in certain cases the application of cold will produce a similar result: thus, during the hybernation of some animals, as the marmot, the temperature is nearly as low as that of the surrounding atmosphere; and when the latter is even somewhat below the freezing point, the heat of the animal being 35°, if a still *greater cold* be added, it acts as a stimulus, producing a temporary increase of heat and vital activity, as is also stated by Dr. Carpenter. "When, however (he says, *Manual*, page 70), the body is exposed to a more intense degree of cold, the animal functions undergo a temporary renewal; for the cold seems to act like *any other stimulus* in arousing them. The respiratory movements and the circulation increase in activity, so as to generate an increased amount of heat." This does not last, however; and the animal at length will die, if left only to its own resources. It must here be quite obvious, that the heat applied to the pigeon, and the cold to the marmot, acted merely as temporary stimuli, and neither gives the slightest support to the theory, that animal temperature is dependant on the fat.

But, that adipose substance does act as a store of nutriment, there can be little doubt; hence, it is well known that the large hump on the camel's back is almost entirely a fatty mass, and that, in his long journies over the desert, where his food is at all times scanty, and often altogether wanting, the hump gradually disappears, till at length only the place where it had been remains. When an Egyptian or an Arab wishes to purchase a camel previous to an expedition, the first thing he does is to examine the state of its hump, well knowing that if it be not sufficiently large, the camel will not be able to bear the starvation of the journey. As to supporting his animal heat, it would be a very superfluous provision, since, in traversing burning sands, under a scorching, unclouded sun, he can require no such unnecessary aid; and I should think, that whatever would *lower* his temperature, could not fail in proving most grateful to his feelings. It is unnecessary to advance more proofs of fat serving as a store of nutriment; and I believe that we may safely attribute the death of Chossat's pigeons to mere starvation, and that the fall of their temperature was not of itself the cause of death, but was merely an effect produced by the weakened state of their circulation, and the general prostration of the vital powers, arising from want of nutriment.

Before quitting this subject, I will advert to two cases of suicide from abstinence, mentioned in the fifth volume of the "British and Foreign Medical Review," page 369, in these words:—

> "The 'American Journal of the Medical Sciences,' for August, 1830, relates the case of a young man, aged twenty-seven, who, in a state of religious melancholy, resolved on thus terminating his life:—He took nothing but water for seven weeks and four days, at the end of which time he died. An Italian, named Viterbi, according to his own statement, unjustly condemned as an accomplice in a murder which was committed, adopted a similar mode of suicide, and died after twenty-one days; during which time, although food and drink were constantly placed before him, he took only one draught of water, and a spoonful of wine."

After reading these cases, I would be much gratified by learning how much fat a young man might have in his composition, who had laboured so long under religious melancholy, that at length he determined on destroying himself by starvation. I would like to know how much fat may, on an average, be "burned off" in a week. I would like to know why the first patient, who drank water, lived four weeks and four days longer than the second, who took only one draught of that liquid during his fatal abstinence. I would farther be gratified in knowing how fat the eagle was, which, as stated by Buffon, being caught in a fox-trap, lived for five whole weeks without food, and did not seem sensibly exhausted, till towards the last week, after which, it was killed, to put an end to its sufferings.* But, above all, I would like to know whether Duncan M'Girdie thought that his mare died from an exhaustion of fat. The only philosopher who has given an account of M'Girdie's experiment is Ensign Evan Dhu Maccombish, and he is silent on that point, his report being only as follows:—"Love will subsist on wonderfully little hope, but not altogether without it. It will be just like Duncan M'Girdie's mare, said Evan, if your ladyships please; he wanted to use her by degrees to live without meat, and just as he had put her on a straw a day, the poor thing died."†

In the seventeenth volume of this "Review," the reader will find a full statement of Chossat's experiments, from which it appears, so far as I can judge, that notwithstanding the numbers of pigeons, turtle-doves, fowls, guinea-pigs, frogs, &c., which were starved to death, to the amount of some hundreds, the results obtained have been perfectly valueless, though we may gather from them one proof of the erroneousness of the modern disintegration theory, which supposes that every exertion of the mind is accompanied by a certain waste of the brain, similar to the supposed disintegration of the muscles, from exertion of the body; or, to use Dr. Carpenter's words, "that every act of the nervous system involves the death and decay of a certain amount of nervous matter, the replacement of which will be requisite in order to maintain the system in a fit state for action."—*Manual, p.* 226. And he tells us farther, that unusual demands upon the powers of the nervous system, "whether by the long-continued and severe exercise of the intellect, by excitement of the emotions, or by the combination of both in that state of *anxiety* which the circumstances of man's condition too frequently induce,—produce an unusual waste, which requires, for the complete restoration of its powers, a prolonged repose."—*Manual, p.* 226.

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* Wood's Buffon, vol. 11, page 62. † Waverley, vol. 3, chap. 7.

Now, if anxiety, or excited emotions, will cause the brain and nerves to waste, what must have been their state in these poor animals, some of which lived till the nineteenth day? They must have long laboured under dreadful anxiety and intense suffering, and had, consequently, enough to excite mental emotion to its utmost stretch, and, therefore, produce extensive disintegration of the nervous system. But did this happen? No; there was scarcely the slightest diminution of the brain and nerves, which *this* Reviewer states as being "the most remarkable of all the results."—*page* 351. But *he also* believes "that *every act* of the nervous system involves a certain disintegration of its *substance*, and a corresponding demand for reparation." Where, then, has he found this means of reparation in the starved animals? Why, in the fat, to be sure; he is obliged to fall back upon it, as his only resource, and observes, *page* 352, that "the maintenance of the normal weight of the brain, and the general disappearance of the fat, seem to indicate that the former is nourished chiefly at the expense of the latter." And thus *my* reviewer's notion, that the fat is a store of combustible matter for keeping up the heat of the system, is fairly thrown overboard; and *this* reviewer stows it snug under hatches, as a reserve for repairing the losses of the brain and nerves. This, however, is only one specimen, among multitudes of its kind, in which all is unstable as water; and I believe firmly that physiology, at the present day, instead of rising to a higher rank in science, is tumbling rapidly to the bottom of the scale. I now take leave of Chossat's experiments, under the firm conviction that they were disgraceful to the man who conducted them; still more so, if possible, to the academy which awarded them a prize, and that they have proved as worthless in their results as they were atrocious and diabolical in their execution.

I must now turn to an earlier part of the Reviewer's notice of the pigmy volume. He denies that "the vital power in any way controls chemical change," and says that "all these peculiar powers, termed vital, are dependent for their manifestation upon chemical changes, which are destructive to the tissues," &c.

Now, I will insist that the possession of vitality *does* prevent chemical change. When various entozoa, or the larvæ of the gad-fly, live and thrive in the stomachs of animals, what preserves them from the action of the gastric juice? Is it not a something pertaining to their living state: a principle, a power, a property—call it what we will—which enables them to resist the chemical action of that fluid? If not, how happens it, that when they die from medicine, or any other cause, this same fluid seizes on them at once, and reduces them to a pulp, as it would any other dead organic matter? Is it not sufficiently obvious, that the living properties of the living animals prevent that chemical action from coming into play, which, after death, their vital principle being gone, is in operation at once? The same argument will apply to the stomach itself, portions of which are so often found dissolved, after death, by the gastric juice, which had no such solvent power over it when living, and possessed of its vital principle. It would be easy to advance many proofs of the existence of such a principle in organic bodies; and there is no impropriety in using the term, though some physiologists of the present day would sweep it, and other terms of similar import, from physiology altogether. The truth, however, is, that though they discard the *name,* they cannot get rid of the *reality ;* and in their pages, however ingeniously argued against, or their denial of it, however supported by special pleading, or mystified by the introduction of matters which have nothing to do with it, still the doctrine of a vital principle is just as prominent as if they adopted all the names that ever were applied to it.

Amongst other objections urged against the use of the term, it is said that there is a danger of its checking inquiry, by our attributing to its agency *alone* the various phenomena observed in organised beings; but it seems to me a most extraordinary notion, that the admission of a living principle should check inquiry into the laws and phenomena of life. I would rather think that there could not be a stronger stimulus to their study. It certainly formed no check to the inquiries of Haller, or John Hunter, or Blumenbach, or—I believe I may safely add—of any one else. As for the idea, that it *alone* is sufficient to explain all the phenomena observable in plants and animals, I would ask, has there ever been a book on Physiology yet written that denied the agency of chemistry and physics in the animal economy ? I can only say that I have never seen, nor ever heard, of such a work.

It has been admitted, time out of mind, that the particles of nutriment absorbed from our food are distributed by the blood to the various parts of the body, and that the new particles take place of the old, which had become worn out, and no longer beneficial, if not injurious, to the system. It was in this way supposed that the human body, at the end of every few years, though the same as to its individuality, was composed of particles different from those by which it had previously been built up; and it was compared, in this way, to the ship 'Argo,' in which Jason sailed in search of the golden fleece, and which, from frequent repairs on the voyage, did not on her return possess one of her original timbers.

That the body wastes, and is renewed again by fresh nutriment, is a fact, I suppose, which no one will dispute ; but it is not the less difficult to understand how the operation takes place. That the blood is the medium by which the nutritive matter is carried through the system, is sufficiently evident; but how are we to explain the method by which from that fluid fat is generated in one place, bone in a second, nervous matter in a third, and muscular fibre in a fourth? Supposing that the blood contains the elements of these, and of all the animal tissues, how is each tissue formed and deposited in its own proper locality? These are questions which cannot be answered; for, in truth, we know nothing farther on the subject than the bare facts.

I have made this digression for the purpose of commenting a little on that part of the above quotation, in which the Reviewer says, that "all these peculiar powers termed vital are dependent for their manifestation upon chemical changes, which are destructive to the tissues exhibiting them." Instead of a slow and gradual change of organic particles, the Reviewer and some others imagine that a rapid disintegration of the several tissues goes on every time these are called into action; and, "in fact," says Dr. Carpenter, speaking of muscular contraction, "it would appear that this *waste* is a *necessary* consequence of the *exercise* of the muscle;—every act of contraction involving the death and decomposition of a certain amount of tissue. And as the presence of oxygen is always necessary for the decomposition of organic substances, so do we find that the penetration of the muscular tissue, by oxygenated blood, is essential to the manifestation of its contractile power."

"Every act of contraction, then, may be said to involve the death of a certain amount of muscular tissue; and the products of decomposition, which consist of the elements of muscular fibre, united with the oxygen of the arterial blood, are carried off by the venous current."—*Manual, page* 212.

In the sentence almost immediately following, we are told that "there are certain muscles, as the heart, and the muscles of respiration, whose action is necessarily constant; and their reparation must take place as unceasingly as their waste."—*Page* 212. Has the author forgotten that in the preceding page or two, he mentions the fact of the frog's heart pulsating for many hours after being separated from the body; that the heart of a sturgeon inflated, and hung up to dry, "has been seen to continue beating until the auricle had become absolutely so dry, as to rattle during its movements," and that Nysten observed the heart of a criminal, who had been executed, contracting sixteen hours and a half after his death? Where was the circulating arterial blood, the disintegration of tissue, and its renewal by the process of nutrition, in these cases? No one of the three was nor could be present; why then did the heart contract so long? Simply because it had not lost the peculiar living principle belonging to the muscular fibre. A still more remarkable example may be found in the 42nd number of the Review, *page* 546, where it is stated, that E. H. Weber "has seen the frog's auricle contracting for sixty hours, and after the blood in which it was placed had become *quite putrid.*" Here, then, we have the heart, though surrounded by dead and putrid blood, retaining its vital principle, and pulsating for sixty hours, by its own inherent living powers.

That the constant decay and restoration of tissue are not necessary to the continuance of contractility in the *voluntary* muscles, we may satisfy ourselves by simply observing a gold-fish in a glass globe of water. It lives months after months without any apparent supply of food, yet the smallest alarm, by day or by night, puts it into strong and vigorous motion.

That a change of the organic molecules, composing the various tissues of the human body, is going on, and must go on while life continues, is plain to be understood,—the old particles being carried off by the excretory and secreting processes, while the new ones are supplied by the circulating blood. But that the sudden decompositions of tissue, supposed to take place through the agency of chemistry, by the Reviewer, and others of his way of thinking, it is difficult to believe.

We have all heard of the Kilkenny cats, which fought so desperately, that, the battle being over, nothing of them could be found but the tips of their tails; but did the disintegration and destruction of muscles spoken of, really take place, during every contraction, their operations would often end in catastrophes scarcely less direful; and many a poor hod-man, or sawyer, or blacksmith, before his day's work could be completed, would be obliged to desist, lest he should be eaten up by his own flesh. It happens, however, that when muscles are kept in a long state of *inaction*, they shrink, lose their bulk, firmness, and strength, and, lastly, almost lose their contractile power; but let them again be brought into action, let them be exercised as before, let them be put on the *disintegrating process*, and they will plump up again, regain their firmness, their contractility, and their strength. Muscles, also, that are frequently brought into violent action, acquire an unusual bulk and power, as has been so often remarked in the arms of the blacksmith, and the legs of the dancing-master; but were the Reviewer's theory of chemical solution true, Taglioni would have been danced off her legs before she was a month upon the stage.

I have already stated that I do not accord with the Reviewer's repeated assertion, that Physiology, at the present day, has acquired a high rank as a science. I believe the very reverse; and that it is daily losing its dignity, its pride of place, and its value, from being burdened with hastily-conceived and groundless theories, not derived from the phenomena of animal life, whether in health or disease, but built up in the laboratory of the chemist, or on the perpetually-recurring mistakes of the micrographer. As respects its union with practical medicine, it formerly was looked up to by the physician as the handmaid of his art; but, in its present state, it is found to be not only unworthy of trust, but to be fraught with danger. Dr. Latham observes, that "it is not all physiology which can be made useful towards the knowledge and treatment of diseases, but only those parts of physiology which are undeniably true, and not only true, but easily, and at once, seen to be so. A great deal of what is termed physiology, has turned out to be a mistake; and so far as it has got mixed up with our notions of disease, (and this has happened to a deplorable extent) it has hindered the progress of practical medicine." * *Another* reviewer, with whom I most cordially agree, says, referring to this passage, "The lamentable delusion that speculative systems of medicine, based upon the ever-changing phantasies of physiology, are calculated to advance the skill of the physician in the management of disease, cannot be too energetically and systematically denounced. Books constructed on this principle are peculiarly dangerous—and dangerous in the direct ratio of their attractiveness, from their prettiness, their superficial ingenuity, and their *apparent* acquaintance with the intricate phenomena of vital action." †

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* Lectures on Chemical Medicine, vol. I, p. 9.

I would not for a moment be thought to insinuate anything derogatory either to the researches of the chemist, or investigation by the microscope. All knowledge, when once ascertained to be certain, must be valuable, though the results to which it may finally lead should not, all at once, be apparent; and I would consider that we owe a debt of gratitude to every man who, in any branch of knowledge, enlarges its positive bounds, even though the *cui bono* should only rest as a matter of uncertainty or conjecture. Some of the greatest discoveries (steam, for instance) have grown up, step by step, from the smallest beginnings, until, at length, they have almost changed the face of the civilised world. But I do object to many of the physiological speculations at present afloat, as being founded on unconfirmed, fanciful, and even contradictory data, while they assume to themselves the explanation of most of the living functions, without taking life itself fairly into the account.

I have now only to notice the last paragraph in the "Review," which is this:—"In a concluding chapter, entitled 'Thoughts on Anatomical Education,' Dr. Drummond urges—what has often been suggested by ourselves as likely to prove advantageous—that the entire first year's course should be of a general preparatory nature, embracing no one subject in detail, but of a nature to lay a broad and solid foundation for the more special inquiries to be subsequently pursued."

Now, in the whole "Review," from its commencement, there is only one paper which bears even a distant resemblance to what is recommended in the last chapter of my puny book:

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† Brit. and For Med. Rev. for July, 1845, page 177.

this is in the tenth volume, where, in an article on medical education, it is recommended, that the student, before he becomes a medical student at all, should, if in his power, attend "a general introductory course of anatomy, which would serve as a preliminary to all subsequent study of the details of medical science."—*Page* 181. But what at once shows the vast difference between this recommendation and that proposed by myself, is, that I speak of anatomy in connexion with the student's *first session of collegiate education,* and advise, that before he should be considered as a student belonging to a second year's course, he should first be qualified, by passing an examination, and thereby proving that he had made a sufficient progress in the first. In the paper alluded to there is nothing of the kind; but, on the contrary, the writer thinks that an attendance on lectures may be dispensed with, as a preliminary step, altogether, and that reading a book on the subject may be sufficient. "We are by no means sure (he says) that *lectures* of this kind are essential," though he does not doubt that they are preferable. "But, by the country student (he adds), who is placed out of the reach of lectures, such knowledge may be gained from a book adapted to communicate it, provided that he avails himself of the opportunities of *post mortem* inspection that may fall in his way, to gain that which *sense* only can convey—more especially if he have the advantage of a judicious instructor." I will not object to this, that country students are not very often in the way of *post mortem* examinations, and have not always good instructors, allowing them to have any at all; but whether or not, it leaves to the student a discretionary power to study, or not, as he may please; whereas the system of preliminary anatomical education I recommended would be imperative. I subjoin it as an appendix, and my readers can judge of its nature for themselves.

This last paragraph of the "Review" is, after all, the most curious. First, we are told that I had urged what had *often* been suggested by the Reviewers themselves. Well, then, I examine the whole of their publication, from its commencement down to this statement, but no such suggestion is to be found. Like Macbeth's witches, all has vanished, and melted, "as breath into the wind," or, in legal phrase, *non est inventus.*

Next, we are advised that the Reviewer's statements (which never existed) recommended, agreeably to my own urging, that the first year's course should "*embrace no one subject in detail.*" I then examine what I did say, and I find that the study of two very extensive subjects, the bones and muscles, *in detail*, form a most prominent feature; that I appropriate nearly sixty lectures for teaching them *in detail;* that students should be examined on them, *in detail*, from day to day, and that they should stand a final examination upon them, *in detail*, at the close of the session.

It is a common remark, that persons of a certain class stand in need of good memories; but I would be unwilling to believe that the Reviewer ranks in that class exactly, and I would rather refer the mistakes he has committed to an unjustifiable carelessness, and to a reckless disregard of the feelings, character, and reputation of others. I now part from him, with a word of advice (and it is a friendly one), that when he next thinks of breaking a fly upon the wheel, he will, before executing judgment, be fully satisfied that he acts upon an honest verdict; and I will advise him farther, for his own sake, so to weigh and examine his premises, in future, as not to run the chance of diverging so very widely, as he has done, from the straight line of TRUTH.

JAMES L. DRUMMOND.

Belfast, October, 1846.

APPENDIX:

BEING THE TWELFTH CHAPTER OF "FIRST STEPS TO ANATOMY," ENTITLED

THOUGHTS ON ANATOMICAL EDUCATION.

It has been stated in the preface to the foregoing observations, that my chief object in publishing them was to supply, in some measure, the loss which anatomical students suffer from not attending the commencement of courses of lectures. But it must be obvious that such means can only be effective in a very minor degree, since more can be clearly comprehended by seeing the specimens themselves, combined with large explanatory drawings and diagrams to assist them, than could be by the most particular description without such aid.

The first year's attendance of a student on medical lectures is often considered to be of such slight importance, that it matters little whether his progress be slow or not; it is considered as a kind of breaking in, an opening merely, to the field of study on which he is entering; and that, should he do little during the first session, all deficiencies can be made up with little difficulty in those which are to follow. This I conceive to be a vital mistake. The first medical session is to the student the most important, perhaps, of all sessions; and if, during it, his studies be slurred over, if he do not acquire habits of attention and *thinking*, if he fall into the lazy apathy of letting what he hears at one ear pass out at the other, if he let his thoughts wander to foreign subjects, instead of keeping them fixed on that treated of by the lecturer, and to which he ought to give his undivided attention, the best that can be said of his acquirements, at the close of the session, will be, that they consist of a heterogeneous mass of ideas, fixed on no solid basis, and connected by no tie of systematic union. In anatomy, he may know a little of the bones, a little of the muscles, a little of the blood-vessels, a little of the viscera, &c. but he knows nothing as he should do; and, having spent his first session in so desultory and fruitless a manner, the probability will be, that his second and succeeding sessions may be passed over in a similar way; so that, at length, his final resource will be, during weeks and months of anxiety and distress, to remain in the hands of a grinder, till he have a *chance* of passing an examination for his degree or diploma.

It would be unfair to insinuate that all students thus mismanage their studies; but no teacher will deny that it is an extremely frequent occurrence, though there are always some young men who, from the first, are sensible of the importance and necessity of making the best use of their time; and it will invariably be found, that those who have honestly studied during the *first*, will continue to do so through every succeeding session, and will always hold a high position over their more careless companions. In the remarks now made, I have no intention of bringing a sweeping accusation against *medical* students, as if *they* were more given to idleness than those of any other description. I believe that the fault lies, not in the student, but in the plan of anatomical education which has been so long pursued; and the proper remedy for the evil would be, I conceive, to approximate, at least for the first session, such education, as nearly as possible, to the method followed with regard to the classics, and the other branches of knowledge taught in our ordinary schools. If a boy commence the Latin grammar, he must first commit to memory the declensions, and understand them well, before he learns the pronouns, and these before he goes on to the conjugations, &c. If he learn arithmetic, he is not permitted to commence subtraction until he has mastered addition, nor to multiplication till he has mastered subtraction, nor to division till he has learned the other three. He must get well acquainted with any one branch before he is allowed to go on to the next. It would be absurd to put him into the second book of Euclid while he remained ignorant of the first. But, in a course of anatomy, the student has, in the progress of the lectures, to listen to the descriptions and details of the most complicated parts of the human structure, when, perhaps, he is most imperfectly acquainted with osteology and myology, which from the very *grammar* of the science, and consequently, he can scarcely understand, and certainly not digest, the greater part of what he hears.

I have just stated, that I consider the bones and muscles as the grammar of anatomy, and I would propose that no one should have the privilege of being enrolled as a second year's student until he had proved, by a close examination, that he had acquired a sufficient knowledge of these during his first. But I would go farther than this, I would make the *entire* first winter's course a *preparatory* one. When we consider that a large majority of medical students commence as mere youths, and that they often remove at once from school to college, it must be plain, that with almost every branch of information connected with medical studies, directly or collaterally, they must necessarily be totally unacquainted, how can it be expected that minds so unprepared will make much certain progress through the numerous and difficult branches of anatomy, constituting a course, as taught in our medical schools, the technical terms of which alone, independent of anything else, require no ordinary degree of attention and memory to secure and retain ?

But, were the young student to go through a *first* course, in which he would be absolutely *obliged* to acquire a competent knowledge of osteology and myology, the two great fundamental roots of the science, and in which he would also have laid before him extended views of the animal economy, not confined to man alone, but embracing every class in the animal kingdom, with what superior advantages would he enter on his *second* course. The groundwork of human anatomy would then have been securely laid, and his mind enlarged by a knowledge of the living functions, as connected with the varieties of structure in the numerous tribes of animated being.

The sort of course I would contemplate with the above views, might be somewhat of the following description. First—general explanations of the principal component materials of the animal frame, such as are given in the preceding chapters, but, of course, more ample in every way, and including the blood, with general observations on the secretions formed from it. Second—the bones to be fully demonstrated, and the students to be closely examined on them from day to day. Third—the muscles to be similarly treated. Fourth—digestion, and its various organs in mammalia, birds, fishes, &c. Fifth—respiration, and the other organic functions, similarly treated. Sixth—the nervous system, organs of sense, &c. &c. I do not, however, pretend to lay down any certain outline of the order in which such a course ought to be conducted, nor of its exact extent; but I would ask whether a first year's course, founded on the principle I suggest, would not be infinitely more advantageous to the student—as giving him comprehensive and enlarged views of the animal economy—as exciting an euthusiasm for knowledge—as imparting extended views of nature, and, at the same time, as laying a sound basis on which to rest his future progress, in acquiring a minute knowledge of the human structure, than any ordinary course of medical anatomy could possibly do? Lectures, embracing a large portion of the subjects treated of in Dr. Roget's Bridgewater Treatise, Jones's General Outline of the Animal Kingdom, &c. would, I am certain, prove to the student of the first year of incalculable advantage.

With regard to the qualification of students for recognition to a second course, I have stated that a proper knowledge of the bones and muscles should be a *sine qua non.* When I say a proper knowledge, I do not mean to include the minuteness that describes every facet of the bones of the carpus or tarsus, the separate origins and insertions of the interossei muscles, or of the deep muscles of the back, such minutiæ being oppressive to the memory which cannot long retain them, and while retained, savouring more of pedantry than use. As to the *other* parts of the course, I would suggest that an examination of a general nature should be held, and that some kind of honorary reward should be conferred on such students as had made the greatest progress, provided, however, that they had previously come fairly through their examination on the bones and muscles.

A difficulty now presents itself, which is this:—one professor could not properly perform the entire duties of an initiatory course, and of another connected with human anatomy alone; and hence two teachers would be requisite—one for first year's students especially, and a second for purely professional objects. With respect to the first, I will suppose that three months would be sufficient for getting through the preliminary lectures, as well as the bones and muscles in detail; and, as the subjects to be treated of, in the remaining three, would partake much of a popular character, this second division of the course (which, like the former, should consist of sixty lectures) might prove available to non-medical as well as to medical students.

However crude these few observations may be, I am certain that the principle they involve is one of great importance, and that its adoption, in some form, would be an act of justice to the student, and be ultimately useful to the public and the profession. It is easier, however, to propose plans, than to bring them into operation; and however desirable I, as an individual, may consider that now suggested to be, it could only be realized by a board, under legal sanction, which would lay down a regular curriculum of medical education for all future medical students in the united empire; or else, by an agreement among the different colleges, to make this curriculum uniform in all, and of a more systematic and defined nature than at present is the case.

Before closing this little work, I would suggest farther, that before any one should be recognised as a medical student at all, he should pass an examination in some well-considered elementary course of school education, prescribed by a competent legal authority, a course not too strict, but sufficiently comprehensive, and so clearly laid down that its scope and extent could neither be misunderstood nor evaded. I wish not to speak harshly, nor uncharitably of any, but I feel bound, in truth, to say, that it is not very unfrequent to meet with medical students who are scarcely acquainted even with the rudiments of their own language. To conclude, it will be observed that I have limited my remarks to the subject of anatomy alone, and I am not prepared at present to hazard an opinion as to what extent they might be applicable to other branches of medical education.